

Scientific ethics and publishing conduct

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Abstract The publishing conduct of scientists is increasingly becoming the object of science-based ethical considerations. Studies have shown that questionable publishing conduct is not a rare phenomenon in this area. National and international scientific organisations are making recommendations about appropriate scientific publishing conduct and, in doing so, are referring to universal ethical principles. A number of science-related ethical norms, for example the prohibition to fabricate or manipulate data, can be justified for all sciences by the goal of knowledge and common epistemological assumptions. On the other hand, other science-related ethical norms, such as rules governing the assignment of authorship and the prohibition of plagiarism, can be justified by means of a science-specific incentive mechanism, which was only explicitly analysed for the first time in detail by Merton (The sociology of science. University of Chicago Press, Chicago, pp. 286–324, 1957/1973): the attainment of *recognition* in the scientific community through *published original articles* on scientific progress. Particularly in conjunction with forms of remuneration based on this, this incentive mechanism leads to the temptation to make one's own contribution to the development of science greater than is actually justified.

Keywords Philosophy of science · Scientific ethics · Publishing conduct · Plagiarism

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1 “Self-plagiarism” or redundant publications: a questionable research practice?

The publishing conduct of scientists is the object of scientific considerations. Thus, for instance, in April 2011, the German Association of University Professors and Lecturers (Deutscher Hochschulverband) (DHV)¹ announced interdisciplinary recommendations on appropriate scientific publishing behaviour (cf. “Appendix”) and referred to the fact that “research work should be cross-border and interdisciplinary based on universal basic ethical principles”.² At the same time it called upon the individual scientific disciplines “to reflect and define what denotes correct and incorrect scientific publishing”.³ Section 3 formulates six *science-related ethical publishing recommendations*, including “adequate research and citation behaviour” as well as complete and correct evidence of scientific prior, independently of the authorship. “The rules per se appear self-explanatory, without anyone being aware why they, and not other rules, exist” (Weingart 1998, p. 13). And this article is all about the “Why?”.

The ethical principles stated above can firstly be interpreted as the science-specific substantiation of a more general *imperative for truth and veracity*⁴ in the publication of scientific results. According to Lenk (1991, p. 56), *scientific responsibility* incorporates “consideration of the rules of legitimate scientific work and fair competition to achieve the best possible objective search for and assurance of truth”.⁵

Adherence to these imperatives is jeopardised in an environment that attaches ever-increasing significance to career incentives by publications in peer reviewed and highly-ranked specialist journals.⁶ There are also case studies for this in the field of economic and business sciences.

Merkl (2011, p. 1004) criticises—over and above the purely factual aspects—the article by Bigus and Zimmermann (2009) with reference to the authors’ *behaviour with regard to citations*: “What is more, the article by Bigus and Zimmermann

¹ Cf. German Association of University Professors and Lecturers (Deutscher Hochschulverband) (2011). All German quotations have been translated by a professional translation service firm.

² German Association of University Professors and Lecturers (DHV) (2011), Preamble I.

³ German Association of University Professors and Lecturers (DHV) (2011), Preamble I.

⁴ This is also emphasised in the resolution passed at the DHV’s 50th Association of University Professors and Lecturers’ Conference in 2000 under Clause 3: “The basic rules of scientific work are identical in all scientific disciplines. The overriding principle is honesty towards oneself and others. Integrity in the search for truth and in the dissemination of scientific knowledge forms the cornerstone of a university lecturer’s professional training.” A similar formulation can also be found in the Joint Position Paper of the Allgemeiner Fakultätentag (Combined Faculties Association), the Fakultätentag (Faculties Association) and the Deutscher Hochschulverband (German Association of University Professors and Lecturers) (2012), p. 2. With regard to the meaning of and differentiation between veracity and truth as conditions for communication, refer to Nida-Rümelin (2011), p. 59 et seq.

⁵ For further aspects of research ethics, refer to Graumann (2006). Resnik (1998/2005) delivers a comprehensive set of values for scientists (e.g. honesty, diligence, transparency) in chapter IV.

⁶ For more detailed information on the change of behaviour caused by this, i.e. a displacement of intrinsic motivation by external incentives, refer to Kieser (2010). Refer also to the critical consideration by Münch (2011) on the changed academic framework conditions.

(2009) is largely based on the content of a dissertation written by Zimmermann and reviewed by Bigus, without this content being disclosed as a source and literal quotations correspondingly being cited as such". Similarly, the article by Quick and Sattler (2011) is based on the results in chapter 12 of the dissertation written by Sattler (2011), which was accepted by the responsible faculty as a dissertation at the beginning of 2010, without this being stated in the article.⁷ The dissertation is not cited in the article and, what is more, key empirical findings of the dissertation that contradict a hypothesis and are therefore extensively discussed, are not (no longer) reported in the article, without reasons being given for this. Without knowledge of the origination and peer review process, this *could* give the appearance in this case, that this was a *particularisation of the truth*,⁸ which would be viewed as an infringement of the German Research Foundation's recommendations. The article by Quick and Sattler (2011) is certainly intended to "report new scientific findings"⁹ however, in doing so, the authors have to consider their obligation to "describe the findings completely" and "give correct and complete references to previous work by the authors and by others (citations)" DFG (1998, p. 62; 2013, p. 82). What is more, this example also teaches us that only strict observation of formal citation rules enables readers, reviewers and publishers to recognise and criticise the retention or varying evaluation of information.¹⁰

There was criticism in the case of the renowned economist Bruno S. Frey that he had submitted an article in collaboration with two co-authors in the Journal of Economic Perspectives (JEP), without advising the publisher that two articles, essentially identical in content, had already been published by these same authors. This conduct resulted in the now public *reprimand* of Frey by the publisher of the JEP David H. Autor: "... we find your conduct in this matter ethically dubious and disrespectful to the American Economic Association, the Journal of Economic Perspectives and the JEP's readers" (Autor 2011). Frey (2011) and his co-author Torgler (both Senior Researchers) accepted their error and apologised for it: "... we well understand your very serious complaint and we both agree that you are right. It was a grave mistake on our part for which we deeply apologise." Further so-called self-plagiarism by the same author are discussed online (in blogs and wikis).¹¹ In fact, the articles published by Frey (2003, 2005) are virtually completely identical, without reference being made in the later published article to the article published 2 years earlier. Essentially only the title was changed, however, ironically, both articles deal with "prostitution" by scientists for the purposes of publication. Strictly speaking, the term "self-plagiarism" is contradictory (an oxymoron) because plagiarism is defined as the appropriation of *other people's* ideas, material

⁷ Refer in this respect to the detailed information by Lenz (2012) and the response with counter-arguments by Quick and Sattler (2012).

⁸ With regard to the ethical problem of "partial truths" in experts' reports, refer to Wandschneider (1991).

⁹ Refer in this respect to Quick and Sattler (2011), p. 316, with details of the study's contribution to findings.

¹⁰ Cf. Lenz (2012, p. 268).

¹¹ Cf. <http://blog.handelsblatt.com/handelsblog/2011/08/29/das-bruno-frey-plag-projekt-bei-google-docs/>; <http://economicsintelligence.com/>.

or words without giving proper credit. However, what is meant is the practice of multiple or redundant publication, i.e. to publish an only slightly modified version of an existing article in another journal without cross-referencing. I define “self-plagiarism” or dual/multiple/redundant publications for the purpose of this article as follows: redundant publication means the author has published a substantial part of the same text and/or data with identical or very similar findings in more than one journal without appropriate justification, permission or cross-referencing (based on COPE 2013, 2009; see also Scanlon 2007; Bretag and Mahmud 2009; Nentjes 2012, pp. 417–419). Missing references to own prior or simultaneously published work is a decisive part of the definition because the author tries to hide the redundancy from editors and readers. Redundant publications may cause a correction or in severe cases a retraction of an published article (COPE 2013, 2009).

Recently, the editors of Research Policy decided to retract two papers written by Ulrich Lichtenthaler, a management professor at the University of Mannheim and one of the most prolific younger business researchers in Germany, because of the existence of several closely related papers by the same author which were not disclosed via citations or other means. A closer examination revealed an inconsistent treatment of variables in the Research Policy papers and the other closely related papers which raised severe doubts with regard to the validity and robustness of the results (for details cf. Research Policy 2012). At January 2014, Retraction Watch counts 13 retractions (<http://retractionwatch.com/category/by-author/ulrich-lichtenthaler/>) for Lichtenthaler, the last one an article in the Academy of Management Journal because of “ethical violations in research practices” (cf. AOM Journal 2013; for details cf. Matlack 2013). The WHU Otto Beisheim School of Management where Lichtenthaler got his doctoral degree and teaching license (Lehrbefähigung) decided in September 2013 to withdraw the teaching licence (WHU 2013).

The issues of “self-plagiarism” and “particular truths” are especially suitable for purely science-related theoretical and ethical analysis, as, due to a lack of legal relevance,¹² violations of standards can only be punished by *moral sanctions*, i.e. naming and shaming (cf. Carbonara 2012, p. 443).¹³ These topics continue to be relevant as there are significant “grey areas”¹⁴ between permissible and impermissible conduct. Priddat (2011, p. 866) maintains: “In certain scientific regions, “self-plagiarism” is the standard. ... However, in practice we have apparently long since had other approaches than the scientific moral wishes to face up to with its criticism of self-plagiarism. ... Here it is about economics and not morals. Anyone who adheres to morals will lose.” This quotation proves that there is a need for clarification from a science-related ethical point of view.¹⁵

¹² It should be mentioned that multiple publications may even result in copyright law infringements. I don’t discuss this aspect, see Schack (2013, p. 97).

¹³ For fundamental reflections about the functions of moral sanctions refer to Lenz (2008).

¹⁴ Cf. Riis (2008), p. 9, for the importance of this category.

¹⁵ See also Nentjes (2012) for a partial defense of (transparent!) self-repetition and the critical comments by Sieberg (2012), Skott (2012) and Carbonara (2012).

This article is structured as follows: following the case studies that introduced the subject in *Chapter 1*, *Chapter 2* goes on to explain how the terms “scientific misconduct” and “questionable research practices” are defined by scientific organisations. The focus is on standards for publishing conduct. *Chapter 3* then clarifies the theoretical scientific understanding of the basic principles of science closely based on work by Schurz (2011, 2014) and examines to what extent the normative ideal image of science-based conduct, as discussed by Merton (1942/1973), is compatible with this. The relationship between the normative ideal image and individual incentives for scientists and assumed actual scientific practice will then be examined in more detail in *Chapter 4*. In particular the increasingly-discussed influence of the “economisation” of research on the conduct of scientists and scientific organisations will be considered. *Chapter 5* will then summarise the results.

2 Scientific misconduct, questionable research practices and publishing

Research or scientific misconduct is defined, in the US even *in a legal sense* for the field of state-funded research,¹⁶ by the European Science Foundation (ESF) and the European Federation of National Academies of Sciences and Humanities (ALL European Academies/ALLEA) as following:

“Research *misconduct* is harmful for knowledge. It could mislead other researchers, it may threaten individuals or society—for instance if it becomes the basis for unsafe drugs or unwise legislation—and, by subverting the public’s *trust*, it could lead to a disregard for or undesirable restrictions being imposed on research.

Research misconduct can appear in many guises:

- *Fabrication* involves making up results and recording them as if they were real;
- *Falsification* involves manipulating research processes or changing or omitting data;
- *Plagiarism* is the appropriation of other people’s material without giving proper credit;
- Other forms of misconduct include *failure to meet clear ethical and legal requirements* such as misrepresentation of interests, breach of confidentiality, lack of informed consent and abuse of research subjects or materials. Misconduct also includes improper dealing with infringements, such as attempts to cover up misconduct and reprisals on whistleblowers;

¹⁶ Office of Science and Technology (2000). Cf. Rennie and Gunsalus (2008) for the history of its development. These definitions are of particular significance for the field of healthcare and are executed by appropriate legal regulations; see (US) Department of Health and Human Services (2005), 42. CFR Part 93, § 93.103. The Office of Research Integrity (ORI) is responsible for monitoring this, with a particular focus on legally binding regulations. Rennie and Gunsalus (2008, p. 42) regard supplementary regulations, for example by universities or other research organisations, as necessary, as this definition “leaves out many actions that we find destructive of good science, and which are not covered by other laws”.

- *Minor misdemeanours* may not lead to formal investigations, but are just as damaging given their probable frequency, and should be corrected by teachers and mentors.

The response must be proportionate to the seriousness of the misconduct: as a rule it must be demonstrated that the misconduct was committed intentionally, knowingly or recklessly. Proof must be based on the preponderance of evidence. Research misconduct should not include honest error or differences of opinion” (ESF/ALLEA 2011, p. 6).

Fabrication, Falsification and Plagiarism are clear violations of the ethos of science (ESF/ALLEA 2011, p. 11). Infringements of these *Category I* norms are very serious and are therefore described by Bedeian et al. (2010, p. 716) as *cardinal sins*. Depending on the jurisdiction of the respective country, they can also be punished by law and not just by means of moral sanctions.

Category II includes *doubtful or science-related ethically questionable research practices*, which it is assumed in the following can essentially only be punished by means of moral sanctions. *Category II* includes, for instance, the following conduct (cf. Bedeian et al. 2010; see also ESF/ALLEA 2011, pp. 12–15):

- Ex-post development or adaptation of hypotheses according to the data set;
- Multiple publication of the same or very similar results without reference to prior publications;
- Dissection of results into the “smallest publishable units” (so-called “salami tactics or slicing”);
- Use of personal relationships to publishers for the purpose of publication;
- Acceptance of authorship without having made a significant contribution, e.g. so-called guest or honorary authorship, authorship based on position (Institute Chairman, Professor, PhD Supervisor etc.)¹⁷;
- Non-disclosure of potential relevant conflicts of interest with regard to publication.

The conduct of a researcher can only be described as ethically doubtful or questionable with regard to implicitly or explicitly formulated science-related ethical norms.¹⁸ This raises questions about the occurrence, justification and sanctioning of these norms.

If the results of the survey by Bedeian et al. (2010) of the members of the faculties of 104 AACSB-accredited Business Schools in the USA are to be believed, then between a minimum of around 60 % and a maximum of around 79 % individual have observed category I conduct, and a minimum of around 79 % (inappropriate authorship) and a maximum of around 92 % (ex-post development of

¹⁷ In the judgement of the German Association of University Professors and Lecturers and the (Combined) Faculties Association, this fundamentally amounts to scientific misconduct, cf. German Association of German University Professors and Lecturers/Faculties Association (2012), Clause. III.10.

¹⁸ “Both the definition of scientific misconduct and the specification for proper scientific practice are based upon principles of scientific integrity. These are principles that all scientific and scholarly researchers and practitioners should observe individually, among each other and toward the outside world” (ESF/ALLEA 2011, p. 10).

hypotheses) have observed category II conduct by other members of their faculties during the previous year. In a recent survey among members of an association of German-speaking economists (Verein für Socialpolitik) 38.4 % of the respondents report that they have observed at least one case of scientific misconduct or questionable research practice in the last 3 years (Necker 2012, p. 274). Dubious research practices could therefore no longer be described as a “rare occurrence” even if it has to be admitted that a precise estimation is difficult.¹⁹

As scientific products are recorded and disseminated in publications and a researcher’s reputation essentially grows through publications, I would like to concentrate in the following sections mainly on category II norms that relate to publishing conduct. These include in particular the areas of redundant or multiple publications, i.e. repetition of a part of a published study with identical or minor new data or of the same or only slightly modified content, without reference to prior or simultaneously published articles or books,²⁰ non-disclosed conflicts of interest (e.g. with research projects financed by third-party funding) and inappropriate authorship (e.g. guest or ghost authorship).²¹ Unlike category I, it is less ambiguous here whether this results in damage to the scientific community. It is also rather debatable in individual cases here, whether there has been an infringement at all, that is to say questions of interpretation carry greater weight. Below are a few examples of this: When is there only a slight and when a significant modification of an article? What differentiates a redundant publication from a legitimate replication with additional tests? Possible *conflicts of interest* are of particular importance. Smith (2008, p. 21, incl. further proof) draws a very sobering conclusion for biomedical research: “In other words, we had evidence by the late 1990s that conflicts of interests had a strong influence on results, were common, and were rarely disclosed”. If this is the case, one should ask why this should be any different in economics and business research, the funding for which predominantly comes from industry.²²

Science-related ethical norms of relevance for category II publishing conduct are generally stated by scientific business institutions. Examples include:

- German Research Foundation (DFG), the central institution for research funding in Germany (DFG 1998, 2013);

¹⁹ See Steneck (2006) for a comprehensive overview of studies about research behaviour in different field of studies.

²⁰ It should be mentioned that this behavior may also have legal consequences because of a infringement of copyright laws depending on the respective jurisdiction.

²¹ Cf. Benos et al. (2005). For information on ethically and possibly legally clearly proven misconduct in connection with scientific publications refer to Horner and Minifie (2011). See also Rieble (2010) who discusses plagiarism in jurisprudence.

²² This is the reason for the recommendation by the German Association of University Professors and Lecturers, the Faculties Association and the Combined Faculties Association that “all the (external) factors which might lead an unbiased observer to suspect that the findings reported in the thesis were not achieved through independent academic work. It would also be useful to indicate whether work on the thesis was assisted by a grant, a scholarship or external funds” (German Association of University Professors and Lecturers/Faculties Association/Combined Faculties Association (2012), Clause III.3). See also Adam (2013) for a critical discussion of the relation between economic interests and research findings.

- Academy of Management Code of Ethics, a network of management researchers (AOM 2006)²³;
- Committee on Publication Ethics (COPE 2013, 2009, 2011), an international association of publishers and publishing houses of peer reviewed journals;
- European Science Foundation (ESF) and the ALL European Academies (ALLEA) (ESF/ALLEA 2011), the European association of research organisations and academies;
- German Association of University Professors and Lecturers (DHV), the body representing the interests of German university lecturers, the Combined Faculties Association and the Faculties Association (DHV 2011; DHV/Combined Faculties Association/Faculties Association 2012);
- Code of Ethics for the Verein für Socialpolitik (2012).

Reference is also made here, by way of example, to the DHV science-related ethical publishing recommendations reproduced in “[Appendix](#)”. Without closer examination, I imply below adequate democratic legitimisation of these norms with regard to the members of the said institutions.

3 Epistemological principles of science and *Robert K. Merton’s ethos of science*

From a *science-related theoretical* point of view, the ethical scientific norms outlined above should be justified with the *goal of knowledge* (=G: discovery of preferably true and substantial (content-rich) statements/theories about a specific domain) and the common *epistemological assumptions* (=E1: minimal realism; E2: fallibility and critical approach; E3: objectivity and intersubjectivity; E4: minimal empiricism; E5: logic in a wider sense.²⁴

The epistemological assumptions E1 to E5, widely accepted at least in all predominantly empirical scientific disciplines, are described as follows by Schurz (2014, pp. 23–25):

Minimal realism (E1)²⁵: This means the acceptance of a reality independent of the subject of cognition in conjunction with a correspondence-theoretical term of truth.

²³ Section 4 Research and Publication in particular is relevant with regard to required publishing conduct.

²⁴ For more detailed information on this characterisation of the common principles of science, refer to Schurz (2014, pp. 16–37); Schurz (2011, pp. 21–39). Keuth (1991, p. 119) argues: “However, there are also values on which science *always* depends, above all the value that is attributed to the *truth*”. See for a detailed discussion about the distinction between these science-internal (epistemic) and external values in the context of the value-neutrality requirement controversy Schurz (2014, pp. 37–44) and Schurz (2013). It should be mentioned that these principles characterize the predominant logic of empirical research but there are other positions within the philosophy of science, i.e. radical constructivism, hermeneutics or critical theory. The relationship between these other methodological concepts and the ethos of science is not the subject of this article. Cf. ESF/ALLEA (2011, p. 9 et seq.) about this “mainstream” characterization of the nature of science.

²⁵ The adjective “minimal” indicates that it is not concerned with understanding all aspects of the reality.

Fallibilism and critical approach (E2): This is the principle fallibility of scientific discoveries and, connected to this, the openness to critical analysis.

Objectivity and intersubjectivity (E3): The assumption of objectivity, i.e. the validity of the truth of a statement independently of the epistemological subject, follows from E1. The *de facto* (uncertain) criteria for ensuring objectivity is known as intersubjectivity. “It is a plausible consequence because if a statement’s truth can be established at all convincingly, then—a least in principle—it must be possible to convince any person of the statement’s truth, provided the person is sufficiently cognitively *competent*” (Schurz 2014, p. 23).

Minimal empiricism (E4): Scientific statements or the consequences of scientific theories (with the exception of formal sciences) must be examined with the aid of empirical observations.

Logic in the wider sense (E5): “Using precise logical methods to introduce terms, to formulate sentences, and to construct correct arguments, is the most effective way to approach the goal of content-rich truth (according to assumptions E1–E4)” (Schurz 2014, p. 24).

According to Schurz (2014) “there follow very plausibly four methodological features (M1–M4), which are common to all empirical sciences in the broad sense” from the above goal of knowledge and the epistemological assumptions. The methodological characteristics can be described briefly as follows²⁶: search for the most substantial (content-rich) hypotheses (M1, comes from the general epistemic goal G); recording of empirical data (observation sentences) (M2, from assumption E4); explanation and prediction of empirical data (M3, from E4 and E5); testing of hypotheses by comparison of data with predicted observations M4; from E4 and E5).

The goal of knowledge and knowledge-based theoretical assumptions are abstract and impersonal principles (Ziman 1996, p. 751) and cannot simply be set alongside with the *personal interests/motives* of scientists. The relationship between technical and moral norms and the general goal of science was described by the sociologist Robert K. Merton in 1942/1973 as follows: “The institutional goal of science is the extension of certified knowledge. The technical methods employed towards this end provide the relevant definition of knowledge: empirically confirmed and logically consistent statements of regularities (which are, in effect, predictions). *The institutional imperatives (mores) derive from the goal and the methods.* The entire structure of technical and moral norms implements the final objective. The technical norm of empirical evidence, adequate and reliable, is a prerequisite for a sustained true prediction; the technical norm of logical consistency, a prerequisite for systematic and valid prediction. *The mores of science possess a methodologic rationale but they are binding, not only because they are procedurally efficient, but because they are believed right and good. They are moral as well as technical prescriptions.*”

Four sets of institutional imperatives—*universalism, communism, disinterestedness, organised scepticism*—are taken to comprise the *ethos of modern science*” (Merton 1942/1973, p. 270; italics by H.L.).

²⁶ Cf. for more detail Schurz (2014, pp. 25–27; 2011, pp. 29–32).

The four central imperatives can be characterised as follows:

Universalism (versus particularistic criteria of truth): Claims to truth are not dependent on personal or social qualities of those who claim them; they must prove themselves with reference to stated impersonal criteria. Talented individuals should be guaranteed free access to science; “caste thinking” should be refused. This norm follows with limitations directly from E3, that is claims to truth are independent of the values and beliefs of an individual epistemological subject or—in a wider sense—of the status of a scientist or a journal in which an article has been published.

Communism (versus individual property rights of scientific discoveries): Scientific discoveries become public goods; they become common property. There are no exclusive property rights relating to the use of the findings of individual producers with the key exception of copyrights, i.e. the recognition of the work of the individual and original contribution to the common fund of science and the naming of the creator when used by others.²⁷ Competitive incentives exist due to the build-up of reputation for original contributions, not via the rights of use of the results of new discoveries (refer in this connection to the following chapter 4). This leads to the encouragement of full and open communication, i.e. no retention of findings for the benefit of personal advantage. “The communism of the scientific ethos is incompatible with the definition of technology as ‘private property’ in a capitalistic economy” (Merton 1942/1973, p. 275).

This norm can be linked to the goal of knowledge G and epistemological assumptions E2 and E3, as individual property rights (information as private knowledge that can be exploited on the market) are not compatible with the general goal of the search for knowledge (only for the sake of knowledge) and the goals of critical and intersubjective inquiry of hypotheses and theories. The “approximation of truth” criterion for progress would then be replaced by the criterion of “usefulness or market success”. “The economisation of science means, at a normative level, that scientific norms are largely replaced by economic norms. In a fictitious end state of development, any scientific value would be definable by the price of the corresponding object on the underlying market; knowledge would be expressed by sentences, for which a positive prize would be achieved in exchange” (Balzer 2003, p. 107). Due to problems with information, in a world like this many scientific discoveries assigned to basic research, would no longer be produced (Balzer 2003, p. 101).

Disinterestedness, selflessness (or more neutral: *Impartiality or neutrality*): “Science ... includes disinterestedness as a basic institutional element. Disinterestedness is not to be equated with altruism nor interested action with egotism. Such equivalences confuse institutional and motivational level of analysis” (Merton 1957/1973, p. 274 et seq.). According to the opinion of Merton, fraud in science is a rather rare phenomenon, because there is strict control by others and accountability

²⁷ “The scientist’s claim to ‘his’ intellectual ‘property’ is limited to that of recognition and esteem which, if the institution functions with a modicum of efficiency, is roughly commensurate with the significance of the increments brought to the common fund of knowledge” (Merton 1957/1973, p. 273).

to other scientists. There is no asymmetrical dissemination of information, as other scientists are also experts. “Cultism, informal cliques, prolific but trivial publications—these and other techniques may be used for self-aggrandizement. But, in general, spurious claims appear to be negligible and ineffective” (Merton 1942/1973, p. 276). This norm can also be justified, with restrictions, by recourse to G, E2 and E3. The invention or falsification of data is, for example, not compatible with the goal of the search for true and substantial theories and their critical examination.

Organised scepticism: We can equate this norm directly with the epistemological assumption E2 “Fallibilism and critical attitude”.

Institutional norms cannot be equated directly with individual motives of scientists, as Merton illustrated by means of the “disinterestedness” norm: “For once the institution enjoins disinterested activity, it is to the interest of the scientist to conform on pain of sanctions and, insofar as the norm has been internalised, on pain of psychological conflict” (Merton 1942/1973, p. 276). This (ideal) academic research culture is named in the version by Ziman (1996) CUDOS (=reputation, prestige): Communalism, Universalism, Disinterestedness, Originality, Scepticism. The criterion of *Originality* specifically includes a characteristic that serves scientific progress and which Merton (1957/1973) himself emphasised at another point (cf. the following chapter 4).

We get as an *intermediate result*: The scientific ethos formulated by Merton can be justified very well with the general goal of science and common epistemological assumptions (see also ESF/ALLEA 2011, p. 10, with reference to Merton).

What relationship now exists between Merton’s scientific ethos and specific science-related ethical publishing recommendations, e.g. the six DHV regulations (2011; cf. “Appendix”)? It is not primarily concerned with the invention or falsification of data here, which is evidently incompatible with the goal of scientific knowledge and contradicts Merton’s postulate of disinterestedness. The aforementioned rules aim to clearly make the “true, actual” creative contribution of the involved individual into scientific masterpiece towards the outside world. It would be possible now to argue that the epistemological central *claims for truth* of published scientific discoveries are not impaired at all by the unlawful assignment of authorships or by plagiarism that infringes copyright and by self-plagiarism per se. The results must prove to be intersubjective, i.e. independent of the specific authors. Why then do the aforementioned publishing recommendations attract so much attention? The cause of this is a science-specific incentive scheme that is explained in the following chapter.

4 Ethos of science, incentives for scientific progress and actual conduct

The ideal image of scientific ethics described by Merton may not be equated to scientific reality. Merton (1957/1973, 1968a, b/1973) himself also intensively studied what drives scientists and where the reward for their work lies. His answer can be summarised as follows: It is the search for *recognition, reputation* in the

scientific community achieved through contributions to scientific progress.²⁸ In this contest for recognition,²⁹ it decisively comes down to being the first who succeeds in making a new and significant discovery for theoretical progress. Merton demonstrates this by means of a number of historically scientific examples. The argument about who can claim the right to be the *first* to have made a discovery is an indicator of the significance of this incentive for scientists. “Indeed, the pattern is so common that the Germans have characteristically compounded a word for it, *Prioritätsstreit*” (Merton 1957/1973, p. 289).³⁰ Moreover, this argument is often not driven out of self-interest by those involved themselves, but by representatives often not directly involved. “Indeed, it is their friends and followers, or other more detached scientists, who commonly see the assignment of priority as a moral issue that must be fought to a conclusion” (Merton 1957/1973, p. 291). Merton regards this as the proof that an institutionalised and accepted norm about intellectual property in the intellectual community is affected (Merton 1957/1973, p. 292 et seq.).

A further indicator is the common practice of *naming* a discovery after its inventor. Examples from economic and business science include: the *Lücke* theorem, *Schmalenbach’s* dynamic balance sheet, the *Ohlson* model, *Gutenberg* production function, *Keynesian* macroeconomics, *Williamson’s* transaction cost economics, *Nash* equilibrium etc. This property right (“Discovery X is linked to name Y”) is “obtained” at the same time as the announcement, availability and acceptance of the discovery by the scientific community. “In short, property rights in science become whittled down to just this one: the recognition of others of the scientist’s distinctive part in having brought the result into being” (Merton 1957/1973, S. 294f.). Further indications of this striving for priority in science include *prizes* for important scientific discoveries, which the discoverers are awarded, the most important of which is, of course, the Nobel Prize.

Merton (1957/1973, p. 293) writes as follows on the relationship between institutional norms and private interests: “When the institution of science works efficiently ... recognition and esteem accrue to those who have been best fulfilled their roles, to those who have made genuinely original contributions to the common stock of knowledge. Then are found those happy circumstances in which self-interest and moral obligation coincide and fuse”. Merton clearly understands that

²⁸ The question as to what precisely is understood by scientific progress is not intended to be the object of this article. This question is answered by Thomas S. Kuhn (1970/1978, p. 381) as follows: “It should be clear that in the end effect the explanation has to be a psychological or sociological one, that is to say the description of a value system or an ideology combined with an analysis of the institutions, by which this system is passed on and implemented. When one knows what scientists positively value, one can hope to understand the problems they have to deal with and the choices they make in certain conflict situations. I doubt that another choice can be found”. At another point he lists five qualities that characterise good theories: factual conformity, freedom from contradiction, breadth, simplicity and fruitfulness Kuhn (1973/1978). However, the weighting of the individual criteria is again dependent on subjective criteria.

²⁹ The German Council of Science and Humanities states: “Competition in the scientific system primarily consists in competition to deliver new research results and for recognition in the scientific community” (Wissenschaftsrat 2011, p. 8).

³⁰ Refer to Hands (2006, 2011) for possible reasons for the absence of conflicts about priority in economics.

too strong an orientation, or even exclusive orientation, towards priority can also have undesirable consequences, e.g. the plagiarism of the ideas of others (Merton 1957/1973, p. 312 et seq., 321 et seq.). However, this is essentially kept track of by institutional controls and by further values in science. The value of *originality* “is only one of a complex set making up the ethos of science—disinterestedness, universalism, organised scepticism, communism of intellectual property and humility being some of the others” (Merton 1957/1973, p. 303).

As the majority of scientists will never make a truly importance discovery in their lifetimes, it has to be asked what motivates this group of people to contribute to the (normal scientific) game of science. Merton (1957/1973) also provides an answer to this question—which is still important today with certain limitations: “For most of us artisans of research, getting things into print becomes a symbolic equivalent to making a significant discovery”. The major importance of personal rankings in economics or business research, i.e. the *Handelsblatt* ranking, is clear evidence of the need to make scientific work measurable, comparable and simply communicable across a large research group.³¹ The VHB journal ranking list JOURQUAL is increasingly used in recruitment to obtain an indication of a person’s performance.³² These ranking values are then problematically equated to an individual’s contribution to scientific progress within a discipline, without having to justify what the actual substance of this contribution actually is.

Merton’s thinking has been taken up and expanded by economists (Dasgupta and David 1994; Stephan 1996; Strevens 2012) although the key finding of Merton—the striving for priority in the field of discoveries—is recognised as a significant and non-market-based incentive mechanism for the production of science as a public property.³³ “Priority creates a privately-owned asset from the very act of relinquishing exclusive possession of the new knowledge. To put it dramatically, priority in science *is* the price” (Dasgupta and David 1994, p. 500). This is a comparably efficient remuneration mechanism, which also partially takes into consideration the external effects linked to scientific discoveries. “The more a scientist’s work is used, the larger are the financial rewards” (Stephan 1996, p. 1206). Equally positive, in terms of efficiency, is the statement by Strevens (2012, p. 194): “Scientists choose among research programs based on their prospects for earning scientific credit—status or reputation—and the reward systems calibrates the allocation of credit so that the choices made lead to a distribution of labour that (roughly) maximises the production of social good”.

Naturally, the interrelationship between individual motivation and collective results and potentially negative consequences of this competition for ranking and position (*winner-takes-all-contest*), e.g. in terms of the allocation of resources, is discussed more strongly in these works than by Merton. These aspects are only pursued here in terms of their link to publishing behaviour.

³¹ Even Merton (1957/1973, p. 316) talks of the number of publications as a “ritualised measure of scientific or scholarly accomplishment”. Cf. Wissenschaftsrat (2011) in general about the problems of assessment and management of research performance.

³² Cf. Kieser (2010, 2012) for criticism of this.

³³ Cf. Stephan (1996), p. 1202 et seq; 1206 et seq.

An emphasis on priority competition combined with increasing external incentives, e.g. performance-based remuneration, which in turn is dependent on publication work and external funding, will lead to the increasing probability of scientific and research misconduct. “There is some evidence that there is an increasing incidence of research misconduct. Pressure to publish, commercialisation, greater competition for funds, more opportunities for instance through the internet, evaluation practices, and the current career system for scientists, may all contribute to this unfortunate development” (ESF/ALLEA 2011, S. 11; for a similar observation cf. Wissenschaftsrat 2011, p. 30).³⁴ Reference has already been made in chapter 2 to the study by Bedeian et al. (2010) in connection with thoughts on the frequency of scientific misconduct within Business Schools. A survey with randomised response techniques of 663 accounting researchers, who had published more than five articles in 30 top journals, by Bailey et al. (2001) on the perception of conscious manipulation of data and/or results by oneself or others, provides evidence that those surveyed believe that around 4 % of their own empirical articles and around 20 % of the empirical articles by other researchers have been manipulated.

Further *anecdotal evidence* can be obtained from comments from editors or members of ethics committees of renowned journals. Schminke (Chair, AOM Ethics Education Committee) summarises the results of a survey of 16 publishers of top management journals as follows: “In all, the editors recounted a wide variety of unethical actions” (Schminke 2009, p. 588). The *Academy of Management* passed a very extensive and detailed Code of Ethics, although a survey of members showed that only 10 % of the members had carefully read this Code of Ethics (Schminke 2009, p. 590). Kacmar (2009) illustrates various principles of the AOM Code of Ethics with the aid of examples. Eden (2010), publisher of the *Journal of International Business Studies*, also reports on a series of ethical problems. These include self-plagiarism, no reference to one’s own related work and no mention of possible conflicts of interest. Albers and Sureth (2012, p. 132) address in an editorial the central question: What is a substantial contribution of a submitted paper? They conclude with some rules for editors and authors, among them: “If the article is based on a concept, model or dataset previously used by the same author for a related research question, then this author has to disclose this in detail”.

The 2010 survey of scientists on the research conditions of professors at German universities (see Table 1) conducted by Böhmer et al. (2011) provides some insights about research misconduct and questionable research practices among German scientists. Those surveyed were asked to state whether they had been confronted by varying forms of scientific misconduct, either by themselves or others, in the last 3 years. This was a representative sampling of around 22,000 professors at German universities. The results in Table 1 show seven categories of scientific misconduct were reported with percentages of between a minimum of 15 % and a maximum of 55 % within a reasonable time period.

³⁴ Cf. for some empirical support Fanelli (2010) who shows that publication pressures increases the probability of positive research results.

Table 1 Scientific misconduct—category formation and distribution in all disciplines and in economic and social sciences

Categories of scientific misconduct	In % all disciplines (<i>N</i> = 2,868)	In % economics and social sciences (<i>N</i> = 521)
A: Falsification/manipulation of data and results	23.3	22.5
B: Illegal statement of authorship	54.6	55.5
C: Concealment/distorted interpretation of findings, missing documentation	35.2	34.9
D: Unauthorised/unreferenced use of ideas/data/ publications	40.1	42.0
E: Unlawful use of resources	22.0	28.2
F: Influence by companies/financiers	15.2	22.6
G: Bias/negligence in assessment	50.4	52.6

Source: from Böhmer et al. (2011); excerpt from Table 21 on p. 151 and data from Figure 121–126 on pp. 152–156

If these findings are to be regarded as being meaningful, then the effectiveness of scientific ethical norms in everyday scientific life appears to be very limited; the gap to *Merton's* ideal image appears large. This raises the question as to what can be done to enforce ethical norms. The picture does not look any better in the fields of economics and social sciences; there are either no significant differences from the overall group or the values for scientific misconduct are significantly higher (as with categories E and F).

At a first glance the figures in Table 1 appear very high and even the German Research Council in a comment about the study expresses some concerns about the results with respect to Good (or Bad) Research Practice (DFG and Königs 2011, p. 7). Due to the broad form of the question (Please consider the behaviour of persons in your organization, in your discipline and in review processes) it can be expected that multiple reporting of the same case distorted the results. Fanelli (2009, p. 9) argues that, “while surveys asking about colleagues are hard to interpret conclusively, self-reports systematically underestimate the real frequency of scientific misconduct”. Therefore, to get a conservative estimation of scientific misconduct or questionable research practices we should look for the results in self-reports. According to the meta-analysis of survey data from Fanelli (2009) approximately 2 % of scientists admitted to have fabricated, falsified or manipulated data or results at least once. The unweighted average admission rate of questionable research practices was 9.54 % (Fanelli 2009, p. 6). An often cited study is Martinson et al. (2005) who asked 3,247 scientists in the USA receiving funds from the National Institutes of Health about certain types of behaviour within the previous three years. With respect to publishing-related behaviour 6 % say that they failed to present data that contradict one's own previous research, 4.7 % were publishing the same data or result in two or more publications, 10 % were inappropriately assigning authorship credit, 10.8 % withhold details of methodology or results in papers or proposals and 15.3 % drop observations or data points from

analyses based on a gut feeling that they were inaccurate (cf. Martinson et al. 2005, p. 737, Table 1). With respect to factual scientific misconduct Honig and Bedi (2012) report that 13.62 % of 279 papers presented at the International Management Division of the Academy of Management Conference 2009 exhibit a significant (5 % or more) percentage of plagiarized text passages.

A recent survey of German economist's results in the following figures (Necker 2012, p. 278 et seq., Table 2): Only 2 % admit copying work or ideas from others without giving credit whereas 28 % sometimes copy their own previous work without citing; 26 % don't accept the latter norm as binding. 65 % admit sometimes not checking the content of quoted references, 55 % are sometimes citing strategically and 18 % sometimes present findings selectively. Interestingly, only 1 % confess giving incorrectly co-authorship to others whereas 23 % report about refused incorrect co-authorship.³⁵ 25 % sometimes do not report results which are incompatible with own results, 28 % present empirical results selectively to confirm author's argumentation and 28 % are searching for control variables to get the desired results. Only 2 % (1 %) admit to data fabrication (manipulation) but 8 % say that the sometimes are using tricks to get better statistical results. A survey (List et al. 2001) among members of the American Economic Association shows that approximately 4 % admit to have falsified research data. According to List et al. (2001, p. 167) "it does not speak well for academia that an estimated 7–10 % of academic economists have (1) submitted a manuscript to two or more journals in violation of journal policy, (2) not given graduate students justified co-authorship, or (3) given unjustified co-authorship to a colleague". The findings presented above—even the conservative results—are worrying because they lead to doubts about the integrity of the results given in publications. It is evident that the falsification or manipulation of data and results or their distorted interpretation contradicts the basic epistemological assumptions presented in chapter 2. In practice there are rather profane motives in place of noble epistemological ideals, i.e. maximising the number of publications in peer reviewed journals. The force presumed by Merton for the critical examination of the results submitted by peer reviews or critical replication studies appears not to be sufficient to counter these forms of scientific misconduct (cf. Stroebe et al. 2012 about the myth of the self-correcting nature of science). Possibly it is therefore worth considering new forms of *accountability* by scientists. "In a profession in which six-figure starting salaries are commonplace and quarter-million dollar packages for successful scholars are no longer surprising, the absence of a formal audit process is truly noteworthy. No other profession comes to mind in which the stakes are so high but the formal, systematic accountability bar is so low as to be nonexistent. Not law. Not medicine. Not accounting. Not engineering. None" (Schminke 2009, p. 586). With respect to fabrication and falsification and questionable research practices like manipulation of data and results the most promising way to promote more accurate results would be to open data, methods and scientific workflow for the respective scientific community (cf. for details and possible restrictions Nosek et al. 2012). According

³⁵ Cf. Böhmer et al. (2011, p. 157 et seq) who show that there are many cases of incorrect authorship assignments.

to ESF/ALLEA (2011, p. 13) principles of good data practice are availability and access: “Research data should be placed at the disposal of colleagues who want to replicate the study or elaborate on its findings”. The Code of Ethics for the Verein für Socialpolitik (2012, para. 1) requires: “Research should be transparent and reproducible. Assumptions should be stated clearly. Empirical work should, legal strictures notwithstanding, make data and methods available for the purpose of replication”. Journal editors could make data availability a precondition for publishing (cf. Albers 2013). This broad form of openness³⁶ can be related to and justified by Merton’s norms “communism” and “organised scepticism” and the related epistemological assumptions (cf. Chapter 3).

Finally I would like to consider the question as to why problems of the *unlawful assignment of authorship* or problems of unauthorised or unreferenced *used of ideas/data/publications* should be sanctioned from an ethical point of view. In the above cited survey (Böhmer et al. 2011), Group D includes the following: repeated publication of prior published data or texts as original work; use of third party ideas without their approval and the publication of excerpts of documents or third party data without an indication of sources. The scientific and ethical condemnation of these practices cannot be justified by stating that the search for true and substantiated theories will be hindered because the validity of the statements made in these publications does not depend on the non-disclosure of the actual author or on a previous publication of the same results. *However, what is disturbed is the aforementioned incentive mechanism immanent in science*: being the first to have made a (smaller or larger) scientific discovery and expecting recognition for it from the scientific community. Work is attributed to scientists by the unlawful assignment of authorship, work which they have possibly not undertaken at all or other scientists, who have earned the right to be listed as authors, miss out on recognition. In the case of so-called *self-plagiarism* (i.e. redundant or multiple publications without cross-referencing) combined with publication-based judgments of work, the work of scientists is *inflated* compared with others who make no use of this form of self-aggrandizement. In this respect, these scientists *unfairly* have a competitive advantage in the contest for recognition. This is the basis of more detailed regulations in the AOM Code of Ethics (2006), such as “No. 4.2.2. *Authorship Credit*”:

4.2.2.1. AOM members ensure that authorship and other publication credits are based on the scientific or professional contributions of the individuals involved.

4.2.2.2. AOM members take responsibility and credit, including authorship credit, only for work they have actually performed or they have contributed.

4.2.2.3. AOM members usually list a student as principal author on multiple-authored publications that substantially derive from the student’s dissertation or thesis”.

These attempts at detailed regulations should be understood as an expression of a general principle which emphasise the value of the contribution of individual scientists to scientific progress. As increasingly articles are compiled by multiple authors, this can go as far as explaining in a footnote the contributions made by the

³⁶ For a radical new model of open scientific communication cf. Nosek and Bar-Anan (2012).

individual authors. Particularly in so-called “Big Science”, the writing of a scientific article equates to producing a film more than writing a novel (Smith 2008, p. 23). This leads some publishers to argue for “contributorship” in place of “authorship”. “Researchers do not have to be sorted into authors and non-authors. Instead, they simply describe what they did” (Smith 2008, p. 23; cf. for details Rennie et al. 1997).

Another recent example is the BuR-Editorial from Albers and Sureth (2012) which focuses on the practice of data-slicing (cf. for details Kirkman and Chen 2011) or multiple publications. They suggest some specific rules, i.e.: “If the article is based on a concept, model or dataset previously used by the same author for a related research question then this author has to disclose this in detail” (Albers and Sureth 2012, p. 132; cf. for same requirement ESF/ALLEA 2011, p. 14). Missing disclosure about previous own work hinders editors and readers to assess the substantial contribution of a new submitted or published article. If journal editors and/or reviewers would regularly use software tools (cf. Albers 2013), it would be relatively easy to detect non-cited own work.

This science-specific incentive mechanism also leads—in particular in conjunction with forms of remuneration based on this—to the temptation to make one’s own contribution to the development of science greater than is actually justified. As there are still no clear uniform standards in many disciplines, e.g. in relation to authorship questions, and measures such as this appear particularly favourable to scientists in view of the risks (compared say with the risk of falsifying or manipulating data), it is no wonder that problematical conduct occurs frequently here. The attainment of advantages to an individual’s reputation by unfair and unsanctioned publishing practices can, under certain circumstances, also involve incorrect decisions about allocation by scientific organisations, generally based on the “Matthew principle” put forward by (Merton 1968a, b/1973).

5 Summary and outlook: what can be done to promote research ethics?

The publishing conduct of scientists is increasingly becoming the object of science-related ethical considerations. Case studies and surveys have shown that scientific misconduct or questionable research practices are phenomena which should be considered in academia. National and international scientific organisations are making recommendations about appropriate scientific publishing conduct and, in doing so, are referring to universal ethical principles. A number of science-related ethical norms, for example the prohibition to fabricate or manipulate data, can be justified for all sciences by the goal of knowledge and common epistemological assumptions. In contrast, other scientific norms, such as the regulation of the statement of authorship and the prohibition of plagiarism, can be justified by means of a specific scientific incentive mechanism, which was only explicitly analysed for the first time in detail by Merton (1957/1973): The attainment of *recognition* in the scientific community through *published original articles* to scientific progress. This incentive mechanism leads—in particular in conjunction with forms of remuneration

based on this—to the temptation to make one’s own contribution to the development of science greater than is actually justified.

According to Weingart (1998, p. 13) the continuing occupation with Code of Conduct is possibly “an indicator of the crisis, but not an appropriate means of overcoming it”. The formulation of a Code of (Publication) Ethics is a necessary first step because it alerts editors, reviewers and readers to important ethical issues but the ethical code is not sufficient to ensure norm-compliant behaviour. With respect to publishing in the current system of scientific publishing the decisive “gatekeepers” are publishers, editors and reviewers of journals. They are in a position which enables them to significantly increase the probability of detection of questionable research practices. Based on a Code of Ethics they can develop more specific and concrete procedural guidelines (cf. COPE 2013, 2009, 2011; Albers 2013 for more detailed suggestions). For example, editors may decide to demand to make data public available as a precondition for publishing, editors may encourage reviewers to be alert to redundant publications and may consider to provide reviewers “with tools to detect related publications” (COPE 2011, p. 4). Furthermore, because the average numbers of authors of an article increases, in this case editors may request a detailed description of the *contribution* of each author to the article which is made available to the readers (cf. Rennie et al. 1997). Some journals are going into this direction (cf. Rennie et al. 2000). Lastly, the disclosure of publication misconduct and the open discussion of questionable research practices (see the introductory cases) may act as a deterrent (cf. Wager 2008, p. 94).

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Appendix: The DHV’s science-related ethical recommendations on publication (2011)

“In addition science-based work is cross-border and inter-disciplinary based on universal ethical principles. Individuals contravene these principle if they consciously or negligently make false statements, infringe the intellectual property of others or damage their research work in another manner. Infringement of the designation of co-authors or colleagues, necessary to comply with copyright and university law, also always constitutes scientific misconduct.

Against this background, the German Association of University Professors and Lecturers recommends that its members adhere to the following cross-disciplinary regulations for appropriate science-related publishing behaviour.

1. An individual's own scientific contribution must be at the centre of every scientific publication. An individual's own body of thought and own research results must be clear.
2. Scientific publications require appropriate behaviour with regard to research and citation. Prior scientific work should be referred to as fully and as correctly as possible independently of the respective authorship.
3. Originality and autonomy are determining quality criteria of a scientific publication. It is for this reason that the citation of an individual's own works should be limited to the extent necessary for the content.
4. All co-authors, but also all colleagues who have made their own scientific contribution to the publication, must be named in scientific publications. In the humanities, this includes an individual's own written articles. Other colleagues should be mentioned by name in an acknowledgement.
5. If it is necessary from a specialist point of view, it should be clear from the order of nomination in scientific publications with several authors and/or scientific involvement (see Point 4) the extent of the contribution of an author or other participant in relation to the total piece of work. In many disciplines, the "first-last-author-emphasis" norm has become accepted, by means of which the main author comes in first position and the creative mind or supervisor in last position. However alphabetical lists or authors' names listed according to the degree of involvement are also appropriate in science. What is essential is to have an externally recognisable and thus transparent system of naming in each individual case.
6. The naming of people who have not made their own contribution to the publication is not permitted and is regarded as scientific misconduct".

References

- Adam M (2013) Interessen in wirtschaftsnaher Forschung—nutzen oder bannen? In: Schurz G, Carrier M (eds) *Werte in den Wissenschaften*. Suhrkamp, Berlin. Translation from: Promoting Disinterestedness or Making Use of Bias? In: Carrier M, Howard D, Kourany J (eds) (2008) *The challenge of the social and the pressure of the practice*. University of Pittsburgh Press, Pittsburgh
- Albers S (2013) Preventing unethical publication behaviour of quantitative empirical research by changing editorial policies. *J Bus Econ*. doi:10.1007/s11573-013-0675-3
- Albers S, Sureth C (2012) Editorial: what is and what is not a substantial contribution. *Bus Res* 5:131–132
- AOM (Academy of Management) (2006) Code of ethics. http://www.aom.org/uploadedFiles/About_AOM/Governance/AOM_Code_of_Ethics.pdf. Accessed 18 Feb 2014
- AOM (Academy of Management) Journal (2013) Retraction: Absorptive capacity, environmental turbulence, and the complementarity of organizational learning processes, Ulrich Lichtenthaler. *Acad Manag J* 56:1830
- Autor DH (2011) Letter to Professor Bruno Frey. *J Econ Perspect* 25: 239 et seq
- Bailey CD, Hasselback JR, Karcher JN (2001) Research misconduct in accounting: a survey of the most prolific researchers' actions and beliefs. *Abacus* 37:26–54
- Balzer W (2003) Wissen und Wissenschaft als Waren. *Erkenntnis* 58:87–110
- Bedeian AG, Taylor SG, Miller AN (2010) Management science on the credibility bubble: cardinal sins and various misdemeanours. *Acad Manag Learn Educ* 9:715–725

- Benos DJ, Fabres J, Farmer J, Gutierrez JP, Hennessy K, Kosek D, Lee JH, Olteanu D, Russell T, Shaikh F, Wang K (2005) Ethics and scientific publication. *Adv Physiol Educ* 29:59–74
- Bigus J, Zimmermann RC (2009) Quasirentenmodell und Honorare für Abschlussprüfungen in Deutschland—eine empirische Analyse. *Zeitschrift für Betriebswirtschaft* 79:1283–1308
- Böhmer S, Neufeld J, Hinze S, Klode C, Hornbostel S (2011) Wissenschaftler-Befragung 2010: Forschungsbedingungen von Professorinnen und Professoren an deutschen Universitäten, iFQ-Working Paper No. 81, March 2011
- Bretag T, Mahmud S (2009) Self-plagiarism or appropriate textual re-use? *J Acad Ethics* 7:193–205
- Carbonara E (2012) Self-plagiarizing prostitutes: limiting the need to repeat oneself?—a comment “On the right to repeat oneself” by Andries Nentjes. *Homo Oeconomicus* 29:441–448
- COPE (Committee on Publication Ethics) (2013) What to do if you suspect redundant (duplicate) publication. <http://www.publicationethics.org/files/Full%20set%20of%20flowcharts.pdf>. Accessed 18 Feb 2014
- COPE (Committee on Publication Ethics) (2009) Retraction guidelines. <http://www.publicationethics.org/files/retraction%20guidelines.pdf>. Accessed 18 Feb 2014
- COPE (Committee on Publication Ethics) (2011) Code of conduct and best practice guidelines for journal editors. http://www.publicationethics.org/files/Code%20of%20Conduct_1.pdf. Accessed 18 Feb 2014
- Dasgupta P, David PA (1994) Toward a new economics of science. *Res Policy* 23:487–521
- Department of Health and Human Services (2005) 42 CFR Parts 50 and 93, public health service policies on research misconduct. *Fed Regist* 70(94):28370–28400
- DFG (Deutsche Forschungsgemeinschaft) (1998) Sicherung guter wissenschaftlicher Praxis—proposals for good scientific practice, Denkschrift Memorandum. Wiley-VCH, Weinheim
- DFG (Deutsche Forschungsgemeinschaft) (2013) Vorschläge zur Sicherung guter wissenschaftlicher Praxis—proposals for safeguarding good scientific practice, Denkschrift Memorandum, supplemented edition. Wiley-VCH, Weinheim
- DFG (Deutsche Forschungsgemeinschaft), Königs RP (2011) Stellungnahme der DFG zur Untersuchung „Forschungsbedingungen von Professorinnen und Professoren an deutschen Universitäten“, Bonn, Juli 2011
- DHV (Deutscher Hochschulverband) (2000) Selbstkontrolle der Wissenschaft und wissenschaftliches Fehlverhalten, Resolution of the 50th German Association of University Professors and Lecturers Conference (DHV) 2000. <http://www.hochschulverband.de/cms1/1532.html>. Accessed 02 Sept 2011
- DHV (Deutscher Hochschulverband) (2011) Wissenschaftsadäquates Publikationsverhalten. <http://www.hochschulverband.de/cms1/877.html>. Accessed 06 Aug 2011
- DHV (Deutscher Hochschulverband), Fakultätentage (2012) Gute wissenschaftliche Praxis für das Verfassen wissenschaftlicher Qualifikationsarbeiten, in English: Joint Position Paper of the *Allgemeiner Fakultätentag* (Combined Faculties Association), the *Fakultätentag* (Faculties Association) and the *Deutscher Hochschulverband* (German Association of University Professors and Lecturers), Bonn, 9th July 2012
- Eden L (2010) Letter from the Editor-in-Chief: scientists behaving badly. *J Int Bus Stud* 41:561–566
- ESF (European Science Foundation), ALLEA (All European Academies) (2011) The European code of conduct for research integrity, March 2011
- Fanelli D (2009) How many scientists fabricate and falsify research? A systematic review and meta-analysis of survey data. *PLoS ONE* 4(5):1–11, e5738, doi:10.1371/journal.pone.0005738
- Fanelli D (2010) Do pressures to publish increase scientists’ bias? An empirical support from US States data. *PLoS One* 5(4):1–7, e10271. doi:10.1371/journal.pone.0010271
- Frey BS (2003) Publishing as prostitution?—Choosing between one’s own ideas and academic success. *Public Choice* 116:205–223
- Frey BS (2005) Problems with publishing: existing state and solutions. *Eur J Law Econ* 19:173–190
- Frey BS (2011) Response from Professor Frey. *J Econ Perspect* 25:240
- Graumann S (2006) Forschungsethik. In: Düwell M, Hübenthal C, Werner MH (eds) *Handbuch Ethik*, 2nd edn. J. B. Metzler, Stuttgart, pp 253–258
- Hands DW (2006) Priority fights in economics: paradox and resolution. *Perspect Sci* 14(2):215–231
- Hands DW (2011) Scientific norms and the values of economists: the case of priority fights in economics. Working Paper. <http://ssrn.com/abstract=1919090>
- Honig B, Bedi A (2012) The fox in the hen house: a critical examination of plagiarism among members of the Academy of Management. *Acad Manag Learn Educ* 11(1):101–123

- Horner J, Minifie FD (2011) Research ethics III: publication practices and authorship, conflicts of interest, and research misconduct. *J Speech Lang Hearing Res* 54(Supplement):S346–S362
- Kacmar KM (2009) From the editors: an ethical quiz. *Acad Manag J* 52:432–434
- Keuth H (1991) Die Abhängigkeit der Wissenschaften von Wertungen und das Problem der Werturteilsfreiheit. In: Lenk H (ed) *Wissenschaft und Ethik*. Reclam, Stuttgart, pp 116–133
- Kieser A (2010) Unternehmen Wissenschaft? *Leviathan* 38:347–367
- Kieser A (2012) JOURQUAL—der Gebrauch, nicht der Missbrauch ist das Problem. *Die Betriebswirtschaft* 72:93–110
- Kirkman BL, Chen G (2011) Maximizing your data or data slicing? Recommendations for managing multiple submissions from the same dataset. *Manag Organ Rev* 7:433–446
- Kuhn TS (1970/1978) Logik oder Psychologie der Forschung? In: Krüger L (ed) Thomas S. Kuhn—Die Entstehung des Neuen. Suhrkamp, Frankfurt a.M., pp. 357–388
- Kuhn TS (1973/1978) Objektivität, Werturteil und Theorienwahl. In: Krüger L (ed) Thomas S. Kuhn—Die Entstehung des Neuen. Suhrkamp, Frankfurt a.M., pp. 421–445
- Lenk H (1991) Zu einer praxisnahen Ethik der Verantwortung in den Wissenschaften. In: Lenk H (ed) *Wissenschaft und Ethik*. Reclam, Stuttgart, pp 54–75
- Lenz H (2008) Rationalität, Emotionalität und Moralität—Zur Begründung moralischer Normen, Schmalenbach's Zeitschrift für betriebswirtschaftliche Forschung, special edition 58: 93–115
- Lenz H (2012) Eine wissenschaftsethische Anmerkung zum Beitrag "Beeinträchtigen Beratungsleistungen die Urteilsfreiheit des Abschlussprüfers? Zum Einfluss von Beratungshonoraren auf diskretionäre Periodenabgrenzungen." (erschienen in der zfbf, 63. Jg. (2011), pp. 310–343). *Schmalenbach's Zeitschrift für betriebswirtschaftliche Forschung* 64:262–270
- List JA, Bailey CD, Euzent PJ, Martin TL (2001) Academic economists behaving badly? A survey on three areas of unethical behavior. *Econ Inquiry* 39:162–170
- Martinson BC, Anderson MS, de Vries R (2005) Scientists behaving badly. *Nature* 435:737–738
- Matlack C (2013) Research Fraud Allegations Trail a German B-School Wunderkind. <http://www.businessweek.com/articles/2013-06-24/research-fraud-allegations-trail-a-german-b-school-wunderkind>. Accessed 30 Jan 2014
- Merkel G (2011) Einflussfaktoren der Prüfungshonorare—Anmerkungen zu den Beiträgen "Quarantenenmodell und Honorare für Abschlussprüfungen in Deutschland—eine empirische Analyse" und "Prüfungshonorare in Deutschland—Determinanten und Implikationen". *Zeitschrift für Betriebswirtschaft* 81:1003–1019
- Merton RK (1942/1973) The normative structure of science. In: Merton RK (ed) *The sociology of science*. University of Chicago Press, Chicago, pp. 267–278
- Merton RK (1957/1973) Priorities in scientific discovery. In: Merton RK (ed) *The sociology of science*. University of Chicago Press, Chicago, pp. 286–324
- Merton RK (1968a/1973) Behavior patterns of scientists. In: Merton RK (ed) *The sociology of science*. University of Chicago Press, Chicago, pp. 325–342
- Merton RK (1968b/1973) The Matthew effect in science. In: Merton RK (ed) *The sociology of science*. University of Chicago Press, Chicago, pp. 439–459
- Münch R (2011) *Akademischer Kapitalismus*. Suhrkamp, Berlin
- Necker S (2012) Wissenschaftliches Fehlverhalten—ein Problem in der deutschen Volkswirtschaftslehre? *Perspektiven der Wirtschaftspolitik* 13:267–285
- Nentjes A (2012) On the right to repeat oneself. *Homo Oeconomicus* 29:413–431
- Nida-Rümelin J (2011) *Die Optimierungsfalle. Philosophie einer humanen Ökonomie*. Irisiana, München
- Nosek BA, Bar-Anan Y (2012) Scientific utopia I: opening scientific communication. *Psychol Inquiry* 23:217–243
- Nosek BA, Spies JA, Motyl M (2012) Scientific utopia II: restructuring incentives and practices to promote truth over publishability. *Perspect Psychol Sci* 7:615–631
- Office of Science and Technology (2000) Federal policy on research misconduct. *Fed Regist* 65(235):76260–76264
- Priddat BP (2011) Neue Regelstrenge. Selbstplagiate im Focus, *Forschung & Lehre* No. 11:864–866
- Quick R, Sattler M (2011) Beeinträchtigen Beratungsleistungen die Urteilsfreiheit des Abschlussprüfers? Zum Einfluss von Beratungshonoraren auf diskretionäre Periodenabgrenzungen. *Schmalenbach's Zeitschrift für betriebswirtschaftliche Forschung* 63:310–343
- Quick R, Sattler M (2012) Erwiderung auf Hansrudi Lenz: eine wissenschaftsethische Anmerkung zum Beitrag "Beeinträchtigen Beratungsleistungen die Urteilsfreiheit des Abschlussprüfers? Zum

- Einfluss von Beratungshonoraren auf diskretionäre Periodenabgrenzungen". *Schmalenbach's Zeitschrift für betriebswirtschaftliche Forschung* 64:271–279
- Rennie D, Gunsalus CK (2008) What is research misconduct? In: Wells F, Farthing M (eds) *Fraud and misconduct in biomedical research*, 4th edn. Royal Society of Medicine Press, London, pp 29–52
- Rennie D, Yank V, Emanuel L (1997) When authorship fails. A proposal to make contributors accountable. *JAMA* 278(2):579–585
- Rennie D, Flanagan A, Yank V (2000) The contribution of authors. *JAMA* 284(1):89–91
- Research Policy (eds) (2012) Retracted: the role of corporate technology strategy and patent portfolios in low-, medium- and high-technology firms. <http://www.sciencedirect.com/science/article/pii/S004873330800228X>
- Resnik DB (1998/2005) *The ethics of science—an introduction*. Original edition, London 1998. Routledge, London (E-Book)
- Rieble V (2010) *Das Wissenschaftsplagiat—Vom Versagen eines Systems*. Vitorio Klostermann, Frankfurt a.M.
- Riis P (2008) The concept of scientific dishonesty: ethics, value systems, and research. In: Wells F, Farthing M (eds) *Fraud and misconduct in biomedical research*, 4th edn. Royal Society of Medicine Press, London, pp 3–13
- Sattler M (2011) *Vereinbarkeit von Abschlussprüfung und Beratung*. Gabler, Wiesbaden
- Scanlon PM (2007) Song from myself: an anatomy of self-plagiarism. *Plagiarism* 2:57–66
- Schack H (2013) Wissenschaftsplagiat und Urheberrecht. In: Dreier T, Ohly A (eds) *Plagiate Wissenschaftsethik und Recht*. Mohr Siebeck, Tübingen
- Schminke M (2009) Editor's comments: the better angels of our nature—ethics and integrity in the publishing process. *Acad Manag Rev* 34:586–591
- Schurz G (2011) *Einführung in die Wissenschaftstheorie*, 3rd edn. WBG (Wissenschaftliche Buchgesellschaft), Darmstadt
- Schurz G (2013) Wertneutralität und hypothetische Werturteile in den Wissenschaften. In: Schurz G, Carrier M (eds) *Werte in den Wissenschaften*. Suhrkamp, Berlin, pp 305–334
- Schurz G (2014) *Philosophy of science: a unified approach*. Routledge/Taylor & Francis, New York
- Sieberg KK (2012) Comments on Nentjes' "On the right to repeat oneself". *Homo Oeconomicus* 29:433–436
- Skott P (2012) Systematic dangers of self-repetition—a comment on Nentjes. *Homo Oeconomicus* 29:437–440
- Smith R (2008) Ethical issues in the publication process. In: Wells F, Farthing M (eds) *Fraud and misconduct in biomedical research*, 4th edn. Royal Society of Medicine Press, London, pp 14–28
- Steneck NH (2006) Fostering integrity in research: definitions, current knowledge, and future directions. *Sci Eng Ethics* 2:53–74
- Stephan PE (1996) The economics of science. *J Econ Lit* 34:1199–1235
- Strevens M (2012) Economic approaches to understanding scientific norms. *Episteme* 8:184–200
- Stroebe W, Postmes T, Spears R (2012) Scientific misconduct and the myth of self-correction in science. *Psychol Sci* 7:670–688
- Verein für Socialpolitik (2012) *Code of Ethics for the Verein für Socialpolitik* (English Translation)
- Wager E (2008) The promotion of good publication practice. In: Wells F, Farthing M (eds) *Fraud and misconduct in biomedical research*, 4th edn. Royal Society of Medicine Press, London, pp 89–97
- Wandschneider D (1991) Das Gutachtendilemma—Über das Unethische partikularer Wahrheit. In: Lenk H (ed) *Wissenschaft und Ethik*. Reclam, Stuttgart, pp 248–267
- Weingart P (1998) Ist das Wissenschafts-Ethos noch zu retten? *Gegenworte* 2:13–17
- WHU Otto Beisheim School of Management (2013) *Pressemitteilung vom 13. September 2013*
- Wissenschaftsrat (German Council of Science and Humanities) (2011) *Recommendations on the Assessment and Management of Research Performance*, Drs. 1656-11, Halle, 11. November 2011
- Ziman J (1996) Is science losing its objectivity? *Nature* 382:751–754